

that either the morality or the courtesy of the scientific world is likely to be improved by the renewed exertions on their behalf which are about to be made by Mr. Samuel Butler.

GEORGE J. ROMANES

[This correspondence is now closed.—ED.]

WILL it go any way towards calming Mr. Butler's zeal in the cause of literary honesty to remark that at any rate fifteen years ago, and it may have been further back, Mr. Darwin prefixed to "The Origin of Species" a historical sketch of the progress of opinion on that subject? In view of this it is at least very *misleading* on the part of Mr. Butler to quote the first sentence from the edition of 1859, and then to ask: "What could more completely throw us off the scent of the earliest evolutionists?" as if in those days it would have made a pin's difference to him, or any one else whom he includes in the *us*, whether the scent of the earlier evolutionists lay strong or weak in the track. In these days he should know, if he knows anything of the history of opinion, that these predecessors of Mr. Darwin, with their great though varied merits, had been laughed down, and, for all popular estimation, might be said to have disappeared. To have relied in any way on their authority when Mr. Darwin's book was first published might well have increased the mountain of prejudice against his views without in any way relieving the weight of ridicule that lay upon theirs. When the whole scientific world had been stirred to its foundations and when the whole world almost had been roused into paying attention to science by the awakening genius displayed in the new exposition *de rerum naturâ*, then, when it could best be done, Mr. Darwin turned ridicule into renown, and made all who could even remotely claim to have anticipated or shared his views participators of his fame. Not those who scatter seed at random, but those who cultivate it in chosen ground with indefatigable industry and prevailing skill should, I imagine, be considered the chief benefactors of mankind; and in like manner the fancy that may have fluttered uselessly through many brains becomes at last a fruitful hypothesis or a wide-stretching theory when it falls beneath the cultivation of undaunted genius.

T. R. R. STEBBING

Tunbridge Wells, February 7

"Prehistoric Europe"

WILL you kindly allow me a few words in reply to certain statements made by Prof. Dawkins in his notice of my "Prehistoric Europe." I shall not remark on the perplexing confusion which he gravely puts forward as an outline of my general argument further than to say, in all sincerity, that I fail to recognise in it any trace of what that argument really is. The few observations I have to make shall be confined chiefly to questions of fact.

1. Mr. Dawkins states that I ask geologists to believe that the mammaliferous gravels with Paleolithic implements, which overlie the chalky boulder-clay of East Anglia, were covered by an upper and younger boulder-clay, which latter "has been removed so completely that no trace of it is now to be seen." Now I do not believe that the gravels in question ever were covered by boulder-clay, nor have I written anything which could justify Mr. Dawkins in attributing to me an opinion so absurd.

2. The account I have given of Victoria Cave was written after a careful perusal of all that has been said about it, and my proofs were submitted to Mr. Tiddeman, who reported on the explorations; and therefore I have every reason to believe that my description is correct.

3. The so-called Upper Pliocene deposits at Mont Perrier are described in detail by Dr. Julien, who shows that they are truly interglacial, being younger than the great "pumiceous conglomerate" with its striated stones and blocks, and older than the more recent moraines of the same neighbourhood. Dr. Julien remarks: "La période pliocène supérieure doit disparaître de la science." He correlates the interglacial beds of Mont Perrier with those of Dürnten.

4. The lignites of Lefte and Borlezza, according to Prof. Stoppani, who has carefully studied those closely-adjointing districts, belongs without any doubt whatever to the glacial series; and his observations I have confirmed by a personal examination of the ground. They are generally admitted by Italian and Swiss geologists to be on the same horizon as the lignites of Dürnten.

5. I have not asserted the interglacial age of the so-called

Pliocene of Olmo. The newer deposits in the Upper Val d'Arno, which have usually been assigned by palæontologists to the Upper Pliocene, have been shown by Prof. Mayer, after an exhaustive analysis of the evidence (as well stratigraphical as palæontological) to belong to the Pleistocene; and as their mammalian fauna corresponds with the fauna of the lignites of Lefte and Borlezza, I have said that this fact is "significant," meaning thereby that the beds in question may very likely be of the same age as those near Gandino.

6. Mr. Dawkins says that I deal with my subject not with the impartiality of a judge, but as an advocate, and that I have only called those witnesses which count on my side. I am probably as well acquainted with the literature of the subject as my critic, and after many years' careful reading and study must confess that I have not encountered any evidence that contradicts my views. Had it been my fortune to come upon such evidence I feel sure that I should not have been so weak and foolish, or so untruthful as to have ignored it. Doubtless I have met with many forcible statements of opinion by Mr. Dawkins that he does not agree with me; but I may remind him (and not for the first time) that mere expressions of opinion, however emphatic, prove nothing save, as a rule, the sincerity of him who utters them.

7. My critic further ventures the statement that my classification "is based on ice, and ice only." How very far this is from being the case any candid person may see who shall take the trouble merely to run his eye over the "contents" of my book. Geologists rightly refuse to accept classifications which are based upon so narrow a foundation as a single series of phenomena, such, for example, as Mr. Dawkins's attempt to classify the Pleistocene by reference to the mammalia alone—a classification which, while it draws the line that separates Pliocene from Pleistocene at the base of the glacial deposits in England, would carry the same line, in France and Central Europe, through the middle of the glacial series. Or, to put it another way, if we accepted Mr. Dawkins's classification, we should be forced to admit that the Glacial Period attained its climax in France and Central Europe during Pliocene times, but that it did not begin in England until after the Pleistocene had commenced. And this is the classification which, as may be inferred from the tenor of my critic's remarks, I ought to have adopted.

Mr. Dawkins's remarks upon my views in regard to the evidence of climatic changes I am sorry to say I do not understand. All that I am sure of is that he has quite failed to grasp my meaning—that he has attributed to me opinions which I have done my best to refute—in a word, that he has strangely misrepresented me. But I need not attempt to set him right, as those who are sufficiently interested in the matter are not likely, after this repudiation, to accept his travesty for a reliable presentment of my views.

JAMES GEIKIE

Perth, January 7

On Dust, Fogs, and Clouds

A CURIOUS confirmation of Mr. Aitken's theory of fog was brought to my notice a short time ago. A friend of mine residing in Streatham, struck with the perfection of the heating arrangements in American residences, fitted up his house with a similar contrivance. In the basement was a furnace and boiler which warmed pure air that entered from without, and circulated at a regulated temperature throughout the house. A water-pipe that was connected with the boiler became stopped by frost; an explosion ensued, and the house was filled with so-called steam (hot fog, in fact) from top to bottom. Wherever a cold surface (clock faces, metal fixtures, &c.) was found, even in the topmost bed-rooms, the vapour condensed and left behind it black carbon dust. Nowhere else was this dust found.

Again, few persons who have read Mr. Aitken's paper can have noticed the dejected appearance of the late beautiful snow on the first morning of the welcome thaw without thinking of his theory. What on the previous evening was a clean dazzling mass of exquisite white became a sooty speckled heap of dirty snow. As the sparkling crystals liquefied into water which drained away, they left behind the dust and carbon, around which, according to Mr. Aitken, they originally formed, becoming by multiplication molar and visible. In the streets of London the masses of white snow rapidly became, as somebody remarked, like streams of cold *café au lait*. The whiteness rapidly disappeared and left behind mere dirt.

It may interest some of your readers to know that in 1537

Benvenuto Cellini was attracted to Paris from Florence in consequence of the much clearer and more beautiful atmosphere in the capital of France than in Italy! This fact is derived from the artist's autobiography. What a change now! Paris is rapidly becoming as bad as London.

W. H. PREECE

February 5

IN NATURE, vol. xxiii. p. 195, I found an interesting abstract of a paper read to the Royal Society of Edinburgh, December 20, by Mr. John Aitken, showing "that dust is the germ of which fogs and clouds are the developed phenomena." It is not in the least the intention of this letter to diminish the value of the above-mentioned paper and experiments, but I wished to say that already, several years past, the same results were obtained by Messrs. Coulier and Mascart (1875) in France (*Naturforscher*, 1875, p. 400; *Journal de Pharmacie et de Chimie*, série 4, xxii. p. 165).

In my "Théorie cosmique de l'Aurore Polaire," p. 36, I have already pointed out the great importance of these results on the relation between auroræ and clouds and the danger of measuring the height of auroral displays by means of superior cloudy apparitions (p. 35). In fact, if the invisible aqueous vapour is able to reach much higher regions than terrestrial dust, and if auroræ are in close connection with cosmical matter in a state of extreme division, like our theory attempts to prove, this cosmical matter is without any doubt enabled to form aqueous clouds in a much higher than the usual level. Moreover we have already, in 1873, in the German journal *Gaea* (Köln und Leipzig, E. H. Mayer), asked: "Welches wohl die weitere Rolle der Eisen- und anderen Dämpfe sei, welche nach der Verbrennung in den oberen Regionen der Atmosphäre schwebend bleiben und offenbar nach vollständiger Abkühlung einen Niederschlag von fein vertheiltem Eisenoxyd und anderen Stoffen bilden. Sollten diese Theilchen . . . keine Veranlassung geben können zu den von deutschen Beobachtern so oft wahrgenommenen 'Polarbändern,'¹ deren Zusammenhang mit dem Nordlicht schon öfters dargethan ward, aber bisher unerklärt blieb. Noch würden wir hinzufügen können, mit Hinweis auf die Beobachtung Secchi's eines angeblichen Nordlichts bei Tage (NATURE, October 17, 1872), dass auch die bis jetzt ganz unerklärte, eigenthümliche Gestalt der Cirri, mit ihren ganz regelmässigen, auf ein gewisses Gesetz hindeutenden transversalen Verzweigungen, von der Anwesenheit feiner Eisenstaubkerne in den Eisnadeln möglicherweise bedingt ist. Bekanntlich schweben diese Cirri in den höchsten Wolkenregionen."

It will further be generally known that microscopic meteorites have been found in the centre of hailstones (*Comptes rendus*, 1872, p. 683).

H. J. H. GRONEMAN

Groningen (Netherlands), January, 1881

New Cases of Dimorphism of Flowers—Errors Corrected

REVIEWING my notes and drawings of some years ago, I find the following new cases of dimorphism of flowers:—

1. *Syringa persica*, L., cultivated in the garden of the Lippstädter Realschule, is gynomonocious. In the same inflorescence there are found a majority of hermaphrodite flowers of larger size and a minority of female flowers of smaller size. The hermaphrodite flowers are homogamous and short-styled, like *Syringa vulgaris*, L. (H. Müller, "Die Befruchtung der Blumen," p. 340, Fig. 125). The anthers of the female flowers, which are much reduced in size and never contain any pollen, are inserted sometimes above, sometimes beneath, but commonly in the same height with the stigma. In some few of the small-sized flowers the number of the petals is reduced to three.

2. *Stellaria glauca*, L., near Lippstadt, is gynodioecious, like *St. graminea*, L., as described by F. Ludwig (*Bot. Centralblatt*, No. vi. p. 28), some stems bearing small-sized flowers with very reduced anthers of white colour and greatly-developed stigmas, a vast majority of other stems bearing larger-sized proterandrous flowers with anthers of red colour.

3. *Sherardia arvensis*, L., near Lippstadt, is likewise gynodioecious, its hermaphrodite flowers being proterandrous and larger-sized, with a corolla of 3½-4 mm. diameter, its female

flowers possessing a corolla of only 2½-3 mm. diameter, with extremely reduced anthers.

4. *Asperula tinctoria*, L., produces in Thuringia so frequently flowers with only three petals that in those stems examined by myself by far the greatest part of the flowers were three-petaled.

In my book "Alpenblumen" Dr. Focke of Bremen has detected two errors of naming, which immediately ought to be corrected: the flower described and illustrated on p. 171 is not *Empetrum nigrum*, but *Azalea procumbens*, like that of p. 377; *Cerinth*, in pp. 264, 265, is not *major*, L., but *glabra*, Mill = *alpina*, Kit.

HERMANN MÜLLER

Lippstadt

Geological Climates

I HAVE read with much interest and attention the letters that have appeared in recent numbers of NATURE on the subject of "geological climates," and although it must appear presumptuous on my part to do so, I shall endeavour to show that each of the distinguished writers of these letters may be somewhat in error on at least one point, which—if I am right—must materially affect the correctness of the conclusions they have come to.

I think that Mr. Wallace, whilst very justly giving the Gulf Stream and other currents which might exist were certain lands submerged, credit for great influence in ameliorating the rigour of climate, does not take into sufficient consideration the fact that the waters of the Gulf Stream, although warmer, are, in consequence of holding much more salt in solution, heavier than the colder and less saline Arctic current.

Some experiments show, as clearly as anything done on a very small scale can, that two waters brought as nearly as possible to the conditions of the Gulf Stream and the Arctic current do not mingle when simultaneously poured into a long narrow glass trough; the Arctic water invariably taking its place on the surface.

Supposing then that these two currents meet somewhere about latitude 80° or 81° N., the Arctic water flowing south—if my experiments are of any value—will retain its position on the surface and the warm current pass underneath, and thus lose all its heating influence on the air over a Polar area about 1000 geographical miles or more in diameter.

We can have no stronger example of this effect of difference of density of ocean water than is shown by the two currents in and out of the Mediterranean Sea.

In NATURE, vol. xxiii. p. 242, Prof. Haughton says, "The thickness of this ideal ice-cap at the Pole is unknown, but from what we know of the Palæocrysic ice of Banks Land and Grinnel Land must be measured by hundreds of feet, and its mean temperature must be at least 20° F. below the freezing-point of water."

With regard to both the above assumptions—which are in italics—I must beg to disagree entirely with the learned Professor. He appears to consider the so-called Palæocrysic ice as the normal state of the ice at and near the Pole, and as a natural growth by the gradual freezings or increase of a single floe during a series of years; whereas I am of opinion that this mis-called Palæocrysic ice is the result of a number of floes being forced over and under each other by immense pressure caused by gales of wind and currents.

The western and northern shores of Banks and Grinnel Lands are peculiarly well suited for the formation of such ice-heaps, as they are exposed to the full force of the prevailing north and north-west storms, which pile up the ice in a wonderful manner on these shores and others similarly placed, for a distance of miles seaward. The whole of the west shore of Melville Peninsula is so lined with rough ice of this kind that sledging is impossible.

It will wholly depend upon the form of land—if any—at or near the Pole, whether or not any floebergs are there. If there is no land it is probable there will be few or none, as the ice will meet with no great obstruction, as it is driven by winds and currents.

I have no authorities by me that give the thickness of ice formed in one season at or near the winter quarters of any of the Arctic expeditions, except my own in 1853-4 at Repulse Bay, latitude 66° 32' north.

The measurements of the ice—taken at some distance out in the bay where there was very little snow—and the mean temperature of the air are given on next page.

¹ Or "Polarbänder." My daily observations of these phenomena, beginning with the year 1875, are to be found in the German journal *Wochenschrift*, editor, Dr. Hermann J. Klein in Köln.